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incoming excitation till it has reached a certain intensity and then transmits it to the motor roots; and that there is independent connection between the centripetal nerve and each motor root controlling muscles that enter the reflex. The difficulties involved in the older assumption that the order of contraction is fixed by the various rates of central conduction are avoided by supposing that the order depends on a difference of excitability of the structures connecting the sensory and motor roots. The grade of excitability would depend on chemical conditions, which can change quickly and in limited areas, and so produce the variable order of contraction found. The importance of chemical conditions is apparent in the strychnized frog where the differences of the periods of delay for the different muscles are abolished.

Relation de diverses expériences sur la transmission mentale, la lucidité, et autres phénomènes non explicable par les données scientifiques actuelles. CHARLES RICHET. Proceedings of the Society for Psychical Research, Part XII.

This lengthy article of 150 pages with so startling a title, coming from so prominent a scientist, is sure to attract one's attention. In a topic where so much bad method has prevailed, one expects much from a trained scientific thinker. Unfortunately this expectation is doomed to disappointment. M. Richet's application of the theory of probabilities to his results is very shallow, and the nature of his evidence often entirely too subjective. To begin with, his subjects are four hysterical women, for whose honesty we must be satisfied with Prof. Richet's declaration in their behalf. The first test consists in his willing one of his patients to go to sleep when the latter is at a house several hundred yards distant. Upon going to the house he hypnotizes the subject, who then informs him of the time during which he attempted to will her to sleep. The experiment is varied, but the time given by the subject is in Prof. Richet's opinion so often near the truth that chance fails to account for the successes. Again, hundreds of trials are made to transfer a simple drawing from Prof. Richet's mind to that of the subject. A large number of illustrations record the more successful cases, but the new fact that is emphasized is the discovery that the reproduction was almost equally successful when M. Richet himself was unaware of the character of the drawing to be transferred. This leads him to postulate a state of "lucidity" in which mental impressions are possible without the ordinary aid of the senses. Again, he experimented with a group of sixty drawings with normal subjects, and found on the average seven successful "transfers" in two hundred trials, while with his selected subjects he obtained twenty successes in the same number of trials. The subject while in the hypnotic state attempts to describe the disease of a patient, a lock of whose hair she sees; the descriptions are vague and do not impress the unprejudiced reader as at all noteworthy. Experiments in guessing cards were tried, but the number of successes was not above what chance would account for. This only sketches a small portion of this comprehensive study, which must be read in the original with account of precautions and the illustrations of results.

M. Richet enters upon his research with what appears, in the light of a sound logic, an utterly false notion, namely, that chance or a

new force is the only explanation; entirely neglecting the great probability of our having overlooked a natural mode of explanation, such as the effect of unconscious suggestion. Again, he values the mere accumulation of evidence, as opposed to the stringency of the evidence, far too highly; and more important than all, when he comes to rule out the element of chance successes he fails of his purpose entirely. To begin with, the only type of experiment in which the success attributable to chance is exactly assignable is that with the cards, which proves entirely negative. In all other cases the action of chance is only roughly estimated, with a large element of subjectivity; and to judge from this article, M. Richet seems very readily disposed to see a marvel in every unusual event. In that portion of the article dealing with coincidences, the frequent though not the less unpardonable mistake is committed of confusing the chances of an event happening at a time determined upon *before-hand* by a third party, and the calculation of the chances *after* the event, without taking into account the prediction of the occurrence. Finally, the fact that success was obtained when the agent did not know the nature of the drawings is not an argument for "lucidity," but an argument against telepathy, and suggests that the subject succeeded in getting a sufficient idea of the nature of the drawing to obtain three times the normal number of successes.

Hat das magnetische Feld directe physiologische Wirkungen? L. HERMANN. Pflüger's Arch. XLIII, 5 and 6, April 24, 1888, pp. 217-235.

The psychologic interest in this paper centres about the alleged powers of the magnet in hypnotic phenomena. Prof. Hermann attacks the problem from a purely physiological side, aiming to discover whether the presence of a strong magnetic field in any ways influences the behavior of sensitive tissue under ordinary stimuli. After calling attention to the fact that in the literature of the subject one finds only negative results, when the results are trustworthy, he recounts his own experiments, which were directed mainly to four points. (1) Is there any difference in the minimal intensity of an induction shock that will cause the contraction of a nerve-muscle preparation, when that preparation is in a magnetic field and when it is not? (2) Is there any difference in the curve of contraction of such a preparation when placed in a magnetic field and when not? (3) Is there any difference in the minimal rate of stimuli that will produce tetanus under the two conditions? (4) Will the curve of tetanic contraction differ in the two cases? To all these questions, the answer obtained from numerous experiments, made with great precaution, is entirely negative. The magnetic field has absolutely no physiological effect whatever. Basing his position on these and similar results (for animals behave perfectly normally in a magnetic field; microscopic functions continue as usual; placing one's head between the poles of a magnet results in no sensation), he launches a severe criticism against the unscientific proceedings of the "hypnotists" who attribute a marvellous influence to the magnet, under conditions anything but conclusive. He emphasizes the extreme improbability of any such result, and regards all such anti-physiological announcements as utterly untrustworthy and an evidence of nothing but the careless observation of the reporter.